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FABLES, ARMADYLLICS, AND SELF-REINFORCEMENT[1]

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A FABLE: There once lived a Biologist who, perturbed by repeated designations of Armadillos as Reptiles, wrote a brief note to a learned Society. Therein he set forth some reasons why Armadillos belong with *mammalia,* despite such similarities with *reptilia* as armor and tongue structure. His argument was then not only challenged, such Challenge being a legitimate function of that Society, but since he had denied that Armadillos were Reptiles, he was accused of denying the Existence of Armadillos and of Reptiles, and of denying the Importance of protective armor and highly flexible tongues. He and his Like were accused of a Woeful Lack of Learning. They were informed that Reciprocity exists between animal and plant Kingdoms. If the Armadillo-Reptile classification is a Myth, then so, too, is the Independence of Animals and Plants. Not only does the environmental Plant shape the tongues, behavioral Flights, and other Patterns of Bees, by providing nectar deep in its Flower, but so, too, doth the little busy Bee shape the structure, color, and scent of flowering Plants by selecting which she visits. These issues were not what the Biologist's Note had been addressed to, namely that Armadillos were not Reptiles, but Mammals. And it was thus that the term "armadyllic" entered the Language, to designate Arguments which are addressed in the Manner indicated, to Views other than those held.

MORAL: We should be glad that our domain is Psychology, where armadyllics are not raised.

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My task is facilitated by the independent appearance of a recent article by Catania (1975) on self-reinforcement.[2] His approach is centered on the "logic of the concept" and a comparison of reinforcement and self-reinforcement paradigms in the animal laboratory. There are parallels to my article in conclusions, implications, and interpretations; even the student example is similar. His student "must discriminate the adequacy with which the assignment has been completed [before going to a movie] if the language of self-reinforcement is to be appropriate" (p. 197); my terminology involved *evaluation* of the *response requirement* (also see Goldiamond, 1974, p. 28). Catania concludes that "as a consequence" of mislabelling self-discrimination (self-evaluation) as self-reinforcement, the phenomena involved "have not been properly exploited" (p. 198). I commend his article to the reader. Since both articles are available, I shall repeat neither his arguments nor mine, and shall restrict my concluding comments to a few observations.

From among the many statements by Thoresen and Wilbur, I shall note that *among* the "major points" they *attribute to me* are the following:

"[T]he contingencies . . . are presumed to have an automatic effect on behavior . . . without anything intervening between" (Point 3). The statement that two variables are functionally related is simply a statement of relation. It does not imply the absence of other relations,

[1]The research reported was supported by grants from the Illinois Department of Mental Health on Self-control procedures. Views are those of the author.

[2]EDITORS NOTE. Readers may also wish to read A. Bandura, Self Reinforcement: Theoretical and Methodological Considerations. *Behaviorism (in press)*, which article is a critique of Catania (1975).

processes, *etc.* Since I did not imply this, the attribution is armadyllic.

"4. The concept of self-control is a confusing one." There is an armadyllic difference between that attribution and my citation, namely, "the place of *operant reinforcement* in self-control is not clear" (emphasis added.)

With regard to my hospital experience (Goldiamond, 1973, 1976), yes, I hoped for successes and when I obtained them, I felt encouraged and continued on that tack, and when I did not succeed I sometimes tried harder, and sometimes switched to another tack. And I try to teach my patients when to continue and when to switch. However, at no time did I Mickey Mouse promise myself delivery of a behavioral or other goodie that was independently within my grasp, to get myself to do something else first, and call that goodie a reinforcer. Nor do I teach students to teach patients to do this. When I obtained some muscle recovery as a (hoped for) *consequence* of my *efforts* (my sweat soaked through my outer clothing, and big drops splashed on the floor), this was a genuine *contingency.* I would have been crazy to tell myself, "Well, to obtain such exercising, I shall deprive myself of walking so that I can use walking as a reinforcer." There were independent evaluators of whether or not my exercises had strengthened my muscles sufficiently to lock my knee, support my weight, *etc.* My muscle and organ systems are very hard taskmasters who continually evaluate and reinforce or punish my behavior in accord with exacting standards, many of which I have not yet met. Similarly, the skin of the scratching patient was a harsh taskmaster for her. Thoresen and Wilbur state that I "identified the reduction of skin lesions as *the* reinforcing consequence." I wrote: "*One* consequence was a reduction . . ." There were others. It took time and effort to arrange it so that "her behavior *came* under [her skin's] control," as I noted (all emphases added). The case was *cited* only to exemplify genuine contingencies; the program was not under discussion. Further, because I argued that self-reinforcement may belong with self-evaluation (questioning inclusion of armadillos *with* reptiles, but not with mammals) does not mean I "exclude [from consideration] . . . anticipations, self-instructions, and . . . self-evaluation" (denying existence of armadillos *and* reptiles). They *explicitly* abound in my reports of me (1973) and others (1965, 1974).

If Thoresen and Wilbur are responding, to use a terminology that is not mine, to a mental image that mediates between what I wrote and what they write about what I wrote, I find it difficult to know just what Mahoney is responding to.

At the onset, we are informed that I and other operant investigators "may have some homework to catch up on" in the burgeoning fields of biofeedback and self-regulation. It so happens these are pet areas of such investigators, *e.g.,* Whitehead, Renault, and Goldiamond (1975; see explicit reference to biofeedback, Goldiamond, 1973, p. 95), and compare, much earlier, Fischman, Rosenberger, and Goldiamond (1969). Indeed, the self-regulation of biofeedback offers little solace for self-reinforcement. Rather, standard reinforcement contingencies (what T&W call a "narrow" definition) are in effect. An agency independent of the person (a machine) evaluates the adequacy of the person's response before it delivers a beep, or a wave raised above a given line, or what have you. For the other area of needed homework, namely, self-reinforcement, see Catania's analysis of the Mahoney and Bandura data (pp. 194-197). There are a variety of reasons other than ignorance of the literature for not citing it. And neither self-regulation nor self-control was under discussion.

I do not know with what believers in "total environmental control" Mahoney has been arguing, but this is not only armadyllic, it is, to coin another term, an *exhortatory fiction.* (An e.f. is distinguished from a straw man in that straw *exists.* It is chosen for demolition because it is weak . . .

I build my house of straw
Tum-dum dum, tee dum daw.

An e.f., in contrast, is a *fiction* created for the license it provides for demolitional rhetoric). The point is "the central issue" in an e.f. Behavior can be changed by drugs, by surgery, by environmental manipulations, by genetic means, among others. The description of one type of relation *does not preclude the existence of others.*

The independence evaluation of the response requirement which, I maintain, is necessary to define a reinforcement contingency, Mahoney uses as a springboard to proclaim environment-organism interdependence (so what else is new?). However, the discussion raises points about correlations and functional relations that should be clarified. The "independence of events [which] denotes total lack of correlation" and which Mahoney attributes to my views is relevant to the relation between x and y in a correlation equation,

$$\sigma^2_{(x+y)} = \sigma^2_x + 2\rho_{xy}\sigma_x\sigma_y + \sigma^2_y.$$

To the extent that ρ approaches zero, this middle correlational term will drop out, and x and y will be independent. The relation between x and y in a functional relation is given by $y = f(x)$, a different affair. "Behavior," says Mahoney, "may be said to be a function of environment ($B = f(x)$), but environments may also be said to be a function of behavior ($X = f(b)$). *The same data apply to either argument.*" The sentence I have emphasized makes the preceding sentence inadmissible as a *general* statement about functional relations, although it can hold in limited cases. For the same elements in the same sets ("the same data") represented by x and y, $y = f(x)$ does not generally imply $x = f(y)$. The familiar sine curve is expressed by a functional relation, $y = \sin(x)$, or spelled out, *"the sine value of an angle is a function of that angle".* It is simply not true that therefore, we can also state that *the angle is a function of the sine value.* No matter whether the sine curve undulates horizontally or vertically, the range from -1.00 to 1.00 will always be the sine value, and the infinite domain of angles (whether depicted vertically or horizontally) will always be the angle opening. Assignment of independent and dependent variables is not arbitrary. The functional statement $B = f(x)$, does *not* generally imply $x = f(B)$, if x,f, and B are used identically.

A function may be defined as a "collection of ordered pairs, such that no two distinct ordered pairs of the collection have the same first element" (Randolph, 1952, p. 10). Restated, a function is "a rule of correspondence between two sets such that to each element in one set there can be assigned a unique element in the other" (Marks, 1964, p. 72).

In the sine function, for example, for each angle there is a unique sine value, but for each sine value there is no unique angle (*e.g.,* for the angle 0°, there is the unique value 0; for the angle 180°, there is the unique value 0; but for the value 0, there is obviously no unique angle. Hence, *sine value = function (angle),* and *angle ≠ function (sine value).* It can similarly be demonstrated that rates of behavior are functions of different experimenter procedures, and that for the same set of rates and same set of experimenter procedures, the *converse does not hold.* One can, of course, set up *different* relations, whereby the investigator's choice of a schedule is governed by the behavior rates then obtaining, but this is well known, and does not alter the $B = f(E)$ relation in the other case.

It should be noted that the assignment of "a unique element in the other set" to each value in the first does *not* imply that for each element in the other set there is also a unique element in the first set. This is implied by Mahoney's statements, as quoted. Under certain limiting conditions, such equivalence of uniqueness *may* be found (for example, the set of areas of a circle, and the set of its radii), but even in that case, each element in the first set has a unique other element. The general statement permits one-way uniqueness. If Mahoney does not like the heat of "unidirectional" relations, he should get out

of the functional kitchen. The correlational parlor is open to both directions. It is correlation between x and y that can be stated as a dependency of x on y, or y on x, or both on something else. And finally, at this point, when $B = f(x)$, the assignment of x to environmental events does *not* imply that the range of B cannot also be functionally related to a domain in which the independent variables are nonenvironmental. It is armadyllic to charge others with this nonsequitur.

I shall conclude with one final point. Despite the various armadyllics that Goldiamond and his like deny the existence of inner events, and the "mind [sic]", he is on record as having written:

> We can state, using the same criteria whereby we infer these from human behavior, that we can teach animals to be creative, to abstract, to conceptualize, to think, to develop and apply insight to solve new problems (Goldiamond, 1974, p. 24).

The topic of that discussion was programmed instruction (p.i.), which can teach people to *think* along certain lines, and the first major p.i. text was that by Holland and Skinner. And "the dinner bell not only makes our mouth water, it makes us see food," (Skinner, 1953, p. 266). And further heresy to the "devout":

> Under proper conditions, changes in subjective perception may be attributed to changes in the stimulus conditions. . . . To deny use of subjective experiences of an investigator as clues for properly conducted investigation is to impose restraints not found in other areas of science. (Goldiamond, 1962, p. 310).

When the constraining conditions are set up so that $B = f(x)$ describes the relation between x and B, experimental control of behavior is defined "when the experimenter sets x" to obtain a stipulated value of B, and by the same token, self-control is defined "[w]hen the subject himself sets x at that value" (Goldiamond, 1965, p. 853). Stated otherwise, the subject can then be an applied or experimental analyst of his own behaviors, and his subjective experiences are *not to be denied* as "clues for properly conducted investigation," as just noted. Clearly the armadyllic charge that radical behaviorism denies subjective experience is governed by variables other than careful reading of the material.

Apparently, the second academic generation of radical behaviorists is now undergoing some of the treatment accorded to the first. Those of us who have followed the reception accorded positions formulated earlier are familiar with the misinterpretations and attributions of unheld positions that characterized too many reviews (*e.g.*, attributed denial of thinking, of emotions, of experiences, which parallel earlier charges against Darwin (see Gilbert, 1970). These have usually been published separately. In a sense, we should be grateful to the reviewers of my brief statement for the publication of their armadyllics in the same issue as the original. Readers may thereby judge for themselves.

The Biologist returned to work, feeling that notwithstanding the Valiant defense of protective armor, flexible tongues, and the vigorous statements of interdependence and the like, his main point had not been addressed, and Armadillos were still not reptiles.

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